Evaluating Avian Performance Measures and Source–Sink Dynamics across the CALFED Region

Christine Ann Howell

Public Comments

No public comments were received for this proposal.

Initial Selection Panel Review

Proposal Title

#0292: Evaluating Avian Performance Measures and Source−Sink Dynamics across the CALFED Region

Funding:

Do not fund

Initial Selection Panel (Primary) Review

Topic Areas

- Assessment And Monitoring

Please describe the relevance and strategic importance of this proposal in the context of this PSP. How does the proposal address the topic areas identified above? What are the broader CALFED Goals this proposal may meet that are not accounted for in these specific topic areas?

The proposal is to continue monitoring and assessment studies on riparian avifauna, presumably focussed on songbirds, in the Cental Valley. This is not a high priority for the current PSP, although the CALFED ERP has previously invested heavily in restoration and protection of riparian habitats and their wildlife.

The budgets of proposals submitted in response to this PSP are larger, on average, than those submitted to CALFED in previous years. The Science Program is committed to getting as much science per dollar as is reasonably possible. With this commitment in mind, can the proposed budget be streamlined? If so, please recommend and clearly justify a new budget total in the space provided.

See below

Evaluation Summary And Rating.

Provide a brief explanation of your summary rating and any additional comments you feel are pertinent.

This may be revisted for funded in the future, but it is currently not a high priority area for this Science PSP. It is recommended that the current ERP-funded studies be completed and evaluated prior to expanding the time and scope of the work.

Selection Panel (Discussion) Review

fund this amount: $**0**
note:
do not fund

This proposal is responsive to the "assessment/monitoring" component of the Science Solicitation; but the proposal is specific to riparian bird species, which are not a high-priority at this time. Previous reviews identified several technical concerns with this proposal (e.g. site size, target species) that raised questions about future analyses of this data. Concerns were also raised about estimation of population growth(lambda) and the use of principal components analysis(PCA) in evaluating source-sink issues. These questions were mitigated somewhat by the outstanding skills of the project team. A similar study has been funded by CALFED's ERP program and the panel felt that it would not be prudent to allocate additional funds for work on these topics at this time.

Panel Ranking: Do not fund.

Technical Synthesis Panel Review

Proposal Title

#0292: Evaluating Avian Performance Measures and Source–Sink Dynamics across the CALFED Region

Final Panel Rating
`above average`

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The goals of the proposed investigation are to develop, compare, and evaluate avian performance measures for riparian birds at multiple spatial scales. Overall, the goals of the project are timely, important, and very ambitious. By addressing a variety of population and community metrics across gradients in habitat quality (and restoration), the investigators will not only be able to evaluate population dynamics and how restoration influences communities, but will also be able to make recommendations on how to monitor birds in the future. Because different monitoring schemes require vastly different amounts of expenditures, the investigation should act as a justification for how to properly implement monitoring plans. This study is well justified because it can potentially tackle many issues regarding avian communities, conservation issues, and restoration plans. In general, the goals, objectives, and hypotheses are consistent and well described. The authors provide a conceptual model that is informative, although incorporating how the likelihood of different community and population metrics might be influenced in their conceptual model would improve their overall approach by making predictions when, where, and why metrics might differ. The field methods are generally strong; indeed, PRBO has been a leader in standardizing monitoring methods for bird

communities across the nation. However, a few issues were not addressed. First, it is not at all clear why/how they are choosing their sites. Are they choosing sites to span an environmental gradient in riparian habitat quality? Are they randomly selecting sites? This is most important for interpreting the likelihood of their metrics having enough variation to interpret patterns, but it is also important for understanding their inference space. Second, on what species will they focus their demographic monitoring efforts (e.g., nest success, survival, etc.)? How will differences among species be evaluated? Third, how large will these riparian sites be, and can a large enough sample, particularly for nest success and survival, be estimated to compare demographic metrics to distribution/abundance metrics? It is quite likely that different population and community metrics operate at different spatial/temporal scales, complicating direct comparisons among metrics, and this needs to be considered in their framework. Finally, while the reviewers and I applaud correlating various metrics to estimate the relationships among metrics, a series of correlation matrices is likely too simple of an approach. The investigators should consider other approaches that can directly incorporate non-linearity, thresholds, and covariates (especially interactions) that could quantify complexities and formally estimate when metrics are conditional on certain situations. The main concern for reviewers and myself involved the population modeling of lambda (the intrinsic rate of increase or population growth rate) and estimating how different factors influence lambda. The investigators are rather vague in their estimation of lambda, but this is an important parameter that can be estimated using various techniques, many of which use different types of inputs. Estimating the precision of lambda is therefore critical and depends on not only the uncertainty of juvenile survival, but also every other input into the model (adult survival, nest success, re-nesting rate, likelihood of double brooding, etc.). Because of this, simple thresholds are difficult to estimate and depend on how you define the threshold (based on the point estimate? The confidence interval?). Also, will the same survival rates be used across sites for a given species? For how many species have you estimated adult survival rates? The investigators

also lost most reviewers as to how a PCA analysis can accomplish the task of estimating what factors influence lambda. The way the investigators describe their approach, it appears that the PCA will estimate the variation in physical site characteristics, with the top axes explaining the variation across sites. Assuming each site has a value of lambda, the PC loadings can be plotted as a function of lambda at sites, but this will not provide the investigators with what they are attempting to estimate, "the top two or three axes influencing lambda". The investigators can achieve this, but I suggest that they think more about the appropriate statistical approach. Furthermore, as one reviewer noted, the PCA approach might be difficult for managers and conservationists to interpret. The investigators need to strongly consider how to disseminate these (unavoidably?) complex results and apply them to real-world decision making. The reviewers agreed that the products likely to arise from this project are extremely strong and that the investigators have excellent qualifications for carrying out the project. Overall, the budget seems appropriate, but it was unclear to some reviewers why certain tasks required the amount of time described in the proposal. For example, the investigators describe that it will take 2 months to complete analysis on landscape metrics from a GIS layer, even though the layers are already completed. Overall, the proposed project is important and builds on a large body of work by the investigators. The investigators propose addressing cutting-edge questions in applied conservation biology that should provide novel insight into important, but unstudied, issues related to monitoring and assessment. While there are inevitably some difficult issues to resolve in analysis and interpretation of these complex issues (noted above), I do not foresee this to be a major obstacle in the proposed investigation.

Additional Comments:

The goals of the proposed investigation are to develop, compare, and evaluate avian performance measures for riparian birds at multiple spatial scales. Overall, the goals of the project are timely, important, and very ambitious. By

addressing a variety of population and community metrics across gradients in habitat quality (and restoration), the investigators will not only be able to evaluate population dynamics and how restoration influences communities, but will also be able to make recommendations on how to monitor birds in the future. Because different monitoring schemes require vastly different amounts of expenditures, the investigation should act as a justification for how to properly implement monitoring plans. This study is well justified because it can potentially tackle many issues regarding avian communities, conservation issues, and restoration plans. In general, the goals, objectives, and hypotheses are consistent and well described. The authors provide a conceptual model that is informative, although incorporating how the likelihood of different community and population metrics might be influenced in their conceptual model would improve their overall approach by making predictions when, where, and why metrics might differ. The field methods are generally strong; indeed, PRBO has been a leader in standardizing monitoring methods for bird communities across the nation. However, a few issues were not addressed. First, it is not at all clear why/how they are choosing their sites. Are they choosing sites to span an environmental gradient in riparian habitat quality? Are they randomly selecting sites? This is most important for interpreting the likelihood of their metrics having enough variation to interpret patterns, but it is also important for understanding their inference space. Second, on what species will they focus their demographic monitoring efforts (e.g., nest success, survival, etc.)? How will differences among species be evaluated? Third, how large will these riparian sites be, and can a large enough sample, particularly for nest success and survival, be estimated to compare demographic metrics to distribution/abundance metrics? It is quite likely that different population and community metrics operate at different spatial/temporal scales, complicating direct comparisons among metrics, and this needs to be considered in their framework. Finally, while the reviewers and I applaud correlating various metrics to estimate the relationships among metrics, a series of correlation matrices is likely too simple of an approach. The investigators should consider other approaches that can directly incorporate non-linearity,

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describe that it will take 2 months to complete analysis on landscape metrics from a GIS layer, even though the layers are already completed. Overall, the proposed project is important and builds on a large body of work by the investigators. The investigators propose addressing cutting-edge questions in applied conservation biology that should provide novel insight into important, but unstudied, issues related to monitoring and assessment. While there are inevitably some difficult issues to resolve in analysis and interpretation of these complex issues (noted above), I do not foresee this to be a major obstacle in the proposed investigation.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

Evaluating avian performance measures and source-sink dynamics across the CALFED Region

The panel recognized the team as possessing much field experience, with strong ongoing research and demonstrated technical/analytical competence. The proposal addresses the difficult problems of valuing different metrics for describing and studying bird metapopulation dynamics and of estimating restoration success for birds. The proposal presents a good conceptual model that clearly identifies the important metrics being used to describe bird communities. There is a definite need for additional data and analysis in this area. However, the panel expressed some concern regarding the lack of clear demonstration of how the study will be applied to specific problems. Inclusion of a clear example would have strengthened the proposal, particularly since PRBO has already collected much of the data. Some important information was not included, such as identifying target species. There were also some concerns about how and when the results of the study would be synthesized and published. For these reasons the panel ranked the proposal at above average.

Final Ranking: Above Average

Technical Review #1

proposal title: Evaluating Avian Performance Measures and Source–Sink Dynamics across the CALFED Region

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The goals and objectives of this project are clearly stated and internally consistent. Although hypotheses regarding the relationship among avian performance measures could have been generated, they are lacking. For instance, we might expect a positive correlation between abundance and reproduction in pristine sites but a smaller or nonexistent correlation in altered sites or sites with recent restoration. Such relationships are alluded to in the introductory material and in the examples but are not explicitly stated as testable hypotheses. Some manuscript reviewers will want these stated more explicitly. To this reviewer, it does not seem especially problematic that there are not any such a priori expectations stated. This project would be an important step toward synthesizing results of restoration monitoring across the CALFED region. Individual restoration monitoring efforts can tell us little about the overall status of riparian birds in the CALFED region. Translating monitoring data into information about larger-scale population dynamics is a valid and important objective. Furthermore, this project asks the important question of whether extensive monitoring of productivity and survivorship is justified or whether monitoring abundance or species richness, which are much less expensive, can serve as surrogates. These

	ideas seem timely given that monitoring has been occurring for over a decade.
Rating	very good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

Comments	Existing knowledge (or lack thereof) regarding avian population metrics and source-sink dynamics in the CALFED region calls for a study like this. It is entirely justifiable to address source-sink dynamics using avian population parameters to estimate lambda and assess the source or sink status of individual restoration sites, which probably do function like patches in a traditional metapopulation framework. The conceptual model presented is a good overview of the factors affecting avian populations in a restoration framework, but is too simplistic to be informative regarding this proposal. As the project intends to address the relationships among avian population measures, it would have been much more valuable to depict these expected relationships in the conceptual model. This would have helped with the generation of hypotheses or at the minimum, have helped explain why monitoring only certain parameters may be insufficient. Furthermore, physical habitat properties, landscape properties, and properties of vegetation may have different effects on the various measures of avian populations. This notion actually underlies the entire logic of the portion of the proposal that deals with relationships among avian population metrics. For example, birds may respond with increases in numbers but not in increased reproduction because these habitat properties may affect adult numbers and reproduction differently, there may be a time lag between effects on

	reproduction and an eventual change in adult numbers, or a site may be an "ecological trap." Finally, the conceptual model is presented as if biological interactions have no importance in avian population dynamics. Restoration activities might well influence the abundance of predators or parasites, which could be the most important "habitat feature" for avian populations. Here's what I would have liked to see in the conceptual model: 1) How do various habitat characteristics differentially affect abundance/density, nest success, and adult and juvenile survival? 2) How do predation and cowbird parasitism fit into this picture? 3) Are there some factors that might promote, for example, increased species richness but decreased population levels of individual species? (For example, habitat (or microhabitat) diversity at the expense of suitable amounts of habitat for particular species.) Or increased nest success but decreased juvenile survival? The conceptual model should set up the research questions and hypotheses, and also allow the interpretation of results, and I don't believe it can accomplish either task. Thus, the conceptual basis for the proposed research is weaker than it could be. The project seems ready for full-scale implementation given the abundance of available data and the potential to collect more to fill in the gaps. A pilot project is not necessary.
Rating	very good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The approach for determining source-sink status of restoration sites is strong. The

approach as described will meet the objectives of the project for the most part. The approach is feasible assuming that sufficient sample sizes of nests are obtained to estimate nest success. (More on this in the next section.) However, the method chosen for comparing avian population metrics, as written, may be insufficient. Examining a correlation matrix among the population metrics will no doubt yield some insight into relationships among metrics. But what if relationships exist, but are not linear? Then correlations (which test for linear relationships, assuming the common Pearson correlation is used, which is not specified) could mask underlying complexities in the relationships. For example, abundance and nest success (however they are measured; see next section) could be related in such a way that as abundance increases, nest success increases to a point, perhaps as a result of processes that affect both metrics in a similar manner. At higher levels of abundance, however, nest success could be density-dependent, with lower levels of reproduction in sites with high abundance—a result of intraspecific competition for nest sites and food. Correlations will miss hump-shaped patterns of this nature and other nonlinear relationships. Thus, relationships must be analyzed via examination of scatter plots and perhaps nonlinear forms of regression. Furthermore, it appears that the project will use statistical significance as a benchmark for when metrics are sufficiently closely related (and thus, one might assume, interchangeable in terms of monitoring investment). However, significance can of course arise from high sample sizes and might not be the best benchmark for assuming

that one population metric can be used as a surrogate for another. The selection of a suitable threshold for determining the interchangeability of population metrics needs more thought and should have been explored more in the proposal. Finally, it's important to stress that the spatial scales of the various population metrics must match one another for the proposed approach to have meaning. The sizes of study plots are not specified, nor is the response design (see next section), so it is not clear that sample sizes of nests will be sufficient to calculate nest success at the same spatial scale used for collection of point count data.

The proposal to describe the factors affecting lambda in terms of isoclines on a graph of PCA factors raises some questions in terms of its interpretability. This graph will need to be interpreted by scientists and translated for managers and decision makers. Even scientists who note that, for example, PCA factor 1 needs to be above a certain value to achieve a growing population will need to translate the PCA factor into actual habitat characteristics that are under human control. I strongly encourage detailed interpretation of such a graph (as with the proposed correlation matrix) within the text of any reports. The lack of interspecific interactions within the conceptual model is also a shortcoming here. The proposed PCA ignores the intensity of predation and parasitism and thus will be an incomplete accounting of factors affecting lambda. I am uncertain whether the abundance of potential predators of eggs, nestlings, juveniles, and adults, as well as cowbirds (as surrogates for the intensity of

	predation and brood parasitism, respectively) are monitored on restoration sites. If they are, these factors need to be incorporated into the PCA unless a very strong link between these factors and the habitat properties to be included can be shown. Otherwise, factors affecting lambda will be modeled as if in a community ecological vacuum. That said, the elucidation of habitat factors affecting lambda, which restoration practitioners may have more control over than populations of predators and parasites, will be a significant step forward with a more explicit recognition that not all factors affecting lambda are being considered. Despite these shortcomings, the results could be both interesting scientifically and informative to managers and decision makers. The project will demonstrate the utility of comparing population metrics to develop suitable performance measures that could be applied to other geographic regions, with caution. Decision makers will benefit from 1) advice on population metrics most useful to monitor; 2) suggestions of actions that would alter lambda at specific restoration sites; and 3) the availability of online data for comparison among restoration sites.
Rating	good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	Generally, the approach is well described, but documentation of some important details is lacking. For example, what are the sizes of

	nest plots? What is the point count design? The mist-net layout? Even if standard response designs are employed, it would have been preferable for them to be described in the proposal. Further, is density calculated using distance sampling? Mark-recapture methods? Double-observer methods? Or are relative abundance measures used? Most importantly, how is nest success calculated, and at what spatial scale? The size of study plots becomes especially important when assessing whether plot sizes will be large enough to obtain sufficient numbers of nests for nest success calculations. If nests will need to be combined across sites to achieve sufficient sample sizes, then other adjustments will need to be made to examine the relationship among metrics while keeping spatial scale comparable. While other aspects of the proposal seem technically feasible, I would have liked to see some discussion of nest sample sizes and their implications for the proposed approach. I think the project has a good chance of success overall, despite the concerns voiced above. The project is well within the grasp of the authors, who have a wealth of experience in avian monitoring and statistical analysis of monitoring programs. There is likely no more suitable group than PRBO to attack this problem.
Rating	good

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments

#0292: Evaluating Avian Performance Measures and Source–Sink Dynamics across ...

	The entire project relates to the interpretation of monitoring data and its merits are described in other sections of this review. Proposed additional monitoring builds on monitoring already being conducted and funded by CALFED.
Rating	excellent

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The project could be extremely valuable with some modifications as noted above. The proposal states that the data will be made available online and that the techniques for assessing avian performance measures and for modeling lambda will be described in applied scientific journals, for which the authors have chosen outlets well. The proposal explains in detail how the project will contribute to larger data management systems such as BDAT. The outcomes of the project will be interpretable, especially with assistance on the part of the scientists conducting the project. As noted above, some products, such as the correlation matrix and the PCA isocline graph, will need substantial interpretation by scientists.
Rating	excellent

Additional Comments

Comments	

#0292: Evaluating Avian Performance Measures and Source–Sink Dynamics across ...

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The authors are highly qualified to conduct this research. PRBO has a wealth of experience in landbird monitoring in the Central Valley and elsewhere, and the authors have an excellent record of high-quality research and monitoring publications. Given the level of experience, the infrastructure and administrative support necessary for this project is likely in place.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget seems adequate for the work proposed, but perhaps a little excessive in terms of the time necessary for the project leader (roughly 15 months) and others. It doesn't seem to this reviewer that the tasks described should take as much time as is allotted for them. For example, I'm not sure why it would take an experienced biometrician 1.25 months to generate a correlation matrix and analyze variability in population metrics, which is the main product of Task 2.4. Likewise, why does it take two months to calculate landscape metrics when the GIS coverages, as stated in the proposal, are already available? Further, it appears that there is some overlap among the different tasks. As such, there should be some cost savings. I recommend careful consideration of whether the full budget is justified.
Rating	good

Overall

Provide a brief explanation of your summary rating.

Comments	The project seems a valuable approach to synthesizing information collected at disparate restoration monitoring sites and drawing conclusions that will help decision makers improve habitat restoration for landbirds. Some specifics of the proposal require some more detail or an expansion or alteration of the proposed approach. Overall, this proposal is well written and organized, although the justification for and expectations of the relationships among avian population metrics are incomplete. Synthesis of monitoring data of the sort in this proposal is desperately needed to move beyond site-specific recommendations for habitat restoration and toward regional bird population management.
Rating	very good

Technical Review #2

proposal title: Evaluating Avian Performance Measures and Source–Sink Dynamics across the CALFED Region

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	Yes, the goals, objectives, and hypotheses are consistent and clearly stated. The major goal of the work is to assess the potential role of different bird population/community metrics as measures of riparian restoration success. Components of this include expanding the number of sites and number of years of sampling, determining the direction and consistency (spatially and according to restoration status and landscape composition) of statistical relationships among metrics, estimating the population growth rates for selected bird species across different sites, and determining the combinations of local and landscape characteristics that separate source sites (where reproductive success exceeds mortality) vs. sink sites (where mortality exceeds reproduction). The goal in all of this is to develop avian performance measures that can be used to monitor bird populations and assess restoration success in terms of producing habitat that favors avian population increases and community diversity.
	These are timely and important ideas. This project will bring together and assess the results of avian monitoring sites across the Central Valley, many associated with CALFED-sponsored riparian restoration. This project, in particular, will take a more critical look at how to define restoration success in terms of avian metrics, which should help to prioritize metrics

	for use in monitoring. This project will also help determine the importance of local vegetation structure, restoration age, and landscape composition for productive avian communities. This project could lead to significant progress both on a theoretical and on an applied level.
Rating	excellent

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	I believe the time is ripe for an integrative project like this. PRBO has developed a large avian database across the Central Valley and is beginning to be able to investigate the influences of a number of factors and different spatial scales on riparian bird populations and communities. Although we know that landscape effects are often important on bird communities in fragmented landscapes, we don't know enough specifics to offer prescriptions for restorationists. I think the really nice thing about this study will be the attempt to quantify population growth rates and hence determine which sites (and under which conditions) are functioning as sources and which as sinks for selected species. The conceptual models are strong and emphasize (a) the importance of factors at different spatial scales on bird metapopulation dynamics and (b) the range of metrics that can be derived at different spatial scales, based on occurrence, abundance and productivity of individual species and communities of birds. The authors also do a good job of explaining the rationale for performance measures and why avian metrics need to be evaluated to determine (1) the best metrics and (2) the threshold values for determining restoration or conservation success (e.g.,

	self-sustaining populations of key riparian bird species). The selection as a research project is justified.
Rating	excellent

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	Overall, the approach appears to be well-designed and feasible for addressing the project objectives. This project will add to a large existing body of knowledge that has been compiled by PRBO over the last 12 years. More importantly, it will add much needed critical analysis and evaluation of the data already collected and will develop performance measures that can aid restoration and monitoring practice in the future. This, in combination with a currently funded CALFED project (Songbird population responses to riparian management at multiple scales) will yield important information on the effects of restoration age, vegetation structure, and landscape context on bird population and community metrics. In addition, this project will develop novel information that can be used to assess the source/sink status of local populations. This is an innovative approach that can help lead to better understanding of which bird metrics to use to monitor restoration success and the status of riparian bird populations/communities across the Central Valley. Hence, the information should be useful to decision makers.
Rating	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	Overall the project is well-documented and PRBO has a strong track record in terms of scientific credibility and capability. The goals are rather ambitious, but PRBO has an extensive dataset that may be unmatched in any other area of the country for evaluating source-sink population dynamics across a large area. So, if this kind of analysis can be done anywhere, the Central Valley and PRBO's dataset would be the place and dataset on which it can be done. My biggest question regards the approach that the authors will use to determine thresholds of landscape composition, restoration age, vegetation structure, etc. for distinguishing source vs. sink populations. Principal Components Analysis (PCA) is a good data reduction technique for examining the influences of multiple variables, some of which may be strongly correlated with each other. However, it isn't clear to me how PCA can be used to unambiguously separate out the influences of different variables that influence source/sink status and provide discrete thresholds (say in landscape composition) that will be useful to managers. I would think that some kind of logistic regression or classification and regression tree approach would be better suited for deriving thresholds. That being said, the level of statistical expertise of the research team is quite high, so they probably have a better understanding of this approach than I do.
Rating	very good

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

#0292: Evaluating Avian Performance Measures and Source–Sink Dynamics across ...

Comments	Some additional monitoring is proposed, in order to increase overall sample sizes of surveys (particularly for estimates of nesting success) and to sample a broader array of restoration and remnant sites and landscape configurations. As indicated above, this project will add to an enormous dataset assembled by PRBO on Central Valley riparian birds over the last 12 years. The centerpiece of this proposal is the plan to interpret these monitoring data and develop more effective performance measures for use in bird monitoring and riparian restoration.
Rating	very good

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	Products of high value are likely from this project. The authors plan to submit publications to the scientific journals Restoration Ecology and Ecological Applications. Both of these could be high impact papers within the restoration and avian ecology scientific and practitioner communities. In additon this project will add more sites and years and develop new metrics and data tables to a large existing dataset on Central Valley riparian birds. This project will also develop recommendations for management and tools for monitoring of Central Valley riparian birds. I believe that useful and interpretable outcomes will arise from this project.
Rating	excellent

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	PRBO as an organization and the specific investigators listed on this proposal all have a strong track record in the science of monitoring and assessing avian population and community trends. PRBO has carried on high quality work in avian assessment in the Central Valley and elsewhere for many years. As indicated by the authors, PRBO has helped develop many of the standard methodologies used nationally and internationally for monitoring bird populations. One of the investigators, Dr. Nur, appears to be an expert in biostatistics (Dr. Howell also looks to have a strong background). This level of statistical expertise partially alleviates my concerns over statistical approaches. Finally, PRBO appears to have the infrastructure and data management protocols necessary for accomplishing this project, having compiled and managed a large database from past studies.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget appears adequate for the work proposed. All individual expenditures appear reasonable, but the overall price tag may be a little high for a project that focuses on statistical analysis of mostly pre-existing data. About 45% of the budget is for new field

		data collection. Nest monitoring is particularly labor intensive. However, given the necessity to have an ample dataset (with enough nest monitoring plots and sufficient variation in landscape and local habitat conditions) probably makes the expenditure worth it. Much of the rest of the budget covers salary and benefits of staff and overhead (33%). These expenditures are unavoidable for an organization that must support itself with grants.
	Rating	good

Overall

Provide a brief explanation of your summary rating.

Comments	I like this project very much from both a basic and applied science standpoint. I believe that this is innovative, cutting-edge work that could do much to further understanding of the effects of local vegetation structure and landscape configuration on source-sink dynamics of avian metapopulations. The analysis of the statistical relationships among different avian population/ community metrics will help to determine which metrics are most useful and most reliable for assessing restoration success and population trends.

My only concern is some ambiguity, in my mind, as to precisely how the relative effects of landscape context, local vegetation, and restoration practices will be separated and thresholds identified. This is certainly a worthy pursuit and I have great faith in the ability of the investigators. However, I don't completely follow how the approach outlined by the investigators will accomplish this. |
| **Rating** | very good |

Technical Review #3

proposal title: Evaluating Avian Performance Measures and Source–Sink Dynamics across the CALFED Region

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	This is a remarkably well developed proposal. The goals, objectives, and hypotheses are very well stated and internally consistent. Though ambitious and somewhat complicated, the ideas and analyses proposed are very timely and important for future conservation efforts. This is a good example of "cutting edge" science applied in service to important conservation goals and objectives.
Rating	excellent

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

Comments	The study definitely is justified, given the proposed synthesis and evaluation of more than ten years of existing data. The conceptual model shown in Figure 1 is especially helpful and a useful "roadmap" for the proposed work and its relevance.
Rating	excellent

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	I can answer an enthusiastic "Yes" to each of these questions. My only methodological concern is the lack of an explanation for the method used to calculate rate of population growth (lambda), and whether the estimate of lambda will be deterministic (no presentation of standard deviations) or probabilistic (with standard deviations explicitly stated).
	Because calculation of lambda is central to the proposal and essential for identifying population sources and sinks, its method of calculation and the specific input metrics used should be elaborated upon. In addition, the authors do not describe an explicit method of sensitivity analysis for evaluating the effects of potential variability in the input parameters, which could result from measurement or observer error.
	There have been significant refinements to avian point count protocols as originally described by C.J. Ralph and others. In particular, the double-observer method proposed by Nichols et al. (Auk 117(2): 393-408), should be considered for this study. Using Nichols's approach will allow stronger inferences to be made with respect to the apparent responses of birds, as well as providing some information about observer error. Additionally, there have been substantial advances made in estimating species likelihood of occurrence in samples (e.g. point counts) when species are imperfectly detected (i.e. the species is present, but not detected, as opposed to actually not being present in the sample). See MacKenzie et al. Journal of Animal Ecology 73(3): 546-555 (2004) for an introduction. Application of these methods and

	concepts to the proposed project could strengthen the inferences derived from the work.
Rating	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	See above, under Approach. The method for calculating lambda needs to be explicitly described and referenced. While this is a most ambitious project, the team assembled to do the work has exceptional qualifications and capabilities and a well demonstrated track record. Also, the authors need to be a bit more skeptical and balanced in their evaluation of the utility of birds as indicators of environmental health and condition (see 1988 critique ofindicator species by Landres et al. in Conservation Biology 2(4): 316-328). As a group, birds seem satisfactory for assessing restoration efforts in riparian habitats, based upon apparent responses to vegetation structure and composition. However, selection of an additional indicator species assemblage (e.g. nymphal odonates), more proximate to stream health should be considered,in parallel with birds, though not necessarily as part of this project. Monitoring of multiple indicator species assemblages and parameters (both biological and physical) can lead to a better understanding of how the systems under study are responding to disturbances or mitigation efforts over time.
Rating	very good

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	The approach is well designed and uses methods that are well established and tested. The assessment of avian responses to restoration efforts, given different amounts of time since restoration, could be especially valuable in determining when reclaimed sites could be judged to be fully restored.
Rating	excellent

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The project will yield potentially very valuable information for managers. However, the extent to which the information will be used will depend upon how successfully the information is communicated to managers.
Rating	very good

Additional Comments

Comments	The methods by which dissemination of the information is accomplished need to have an evaluation process to determine their effectiveness. The authors express a commitment to adaptive management. One of the fundamental elements of adaptive management is the explicit inclusion of an evaluation step which allows for fine-tuning the management model and recycling through the adaptive management process.

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	This is an exceptionally well qualified team of investigators. Their track record of past performance is quite solid and their contributions to the more formal, peer-reviewed literature are commendable for employees of a not-for-profit organization. Point Reyes Bird Observatory has assembled one of the best teams of avian ecology and conservation researchers on the West Coast, with the possible exception of the research branch of the U.S. Forest Service and some academic centers.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	Yes, considering the complexity and scope of the project, the budget is quite reasonable.
Rating	excellent

Overall

Provide a brief explanation of your summary rating.

Comments	This is perhaps the best written proposal out of the five that I recently have reviewed for the CALFED area of interest.
Rating	excellent